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Formal Insurance and Informal Solidarity: A Behavioral Experiment on the Philippines

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Abstract

This paper analyzes data from a novel field experiment designed to test the impact of two different insurance products on solidarity in risk-sharing groups among rural villagers in the Philippines. Risk is simulated by a lottery, risk-sharing is possible in solidarity groups of three and insurance is introduced via less risky lotteries. Our main hypothesis is that formal market-based products lead to lower transfers among network members. We also test for the persistence of this crowding-out of solidarity. We find evidence for a reduction of solidarity by insurance if shocks are observable. Depending on insurance design, there is also evidence for persistence of this effect even if insurance is removed.

I. Introduction

A large majority of the population in the world's poorest countries is without formal insurance.[1] Shocks such as natural catastrophes, illness, economic crisis, unemployment or crime, just to mention a few, destroy the economic basis of countless households. As a response, informal transfers within networks of friends, neighbors and relatives are important in the management of these income fluctuations, with transfers consisting of e.g. loans, monetary gifts, goods (such as food) or labor. These support schemes allow households to spread the effects of income shocks throughout their network or village. In this sense, the mutual support in case of a shock is an informal insurance mechanism relying on intrinsic

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[1] Besides social security, only between 2.7% (Asia) and 10% (The Americas) of the population in the 100 poorest countries is covered with formal insurance available to the poor (Roth, McCord, & Liber, 2007, pp.15-19).

motivation to act solidary.[2] This intrinsic motivation is based on friendship and kinship, altruism, inequity aversion or reciprocity (Barr and Genicot, 2008).

There is ample evidence for the importance of such mechanisms in developing countries (e.g. compare Morduch, 1999; Fafchamps, 2008) . Also on the Philippines risk-sharing networks play a major role and respondents may raise funds through gifts and loans (Fafchamps & Lund, 2003), where loans are often zero-interest or do not have to be repaid fully (Fafchamps & Gubert, 2007a). However, if other members are also suffering income shocks, it is more difficult for respondents to raise funds via informal ways. Furthermore, mutual insurance does not appear to take place at the village level; instead, households receive help primarily through networks of friends, relatives and those living close (Fafchamps & Lund, 2003; Fafchamps & Gubert, 2007b). Other evidence from developing countries around the globe suggests that informal insurance only smoothes a fraction of income shocks (Townsend, 1994; Morduch, 1999).[3] Moreover, some do not regard these transfers as genuine risk-sharing. Platteau (1997) for example argues that donors in fact often expect a return for their payment instead of truly internalizing the spirit of mutual insurance.

These imperfections and drawbacks of informal mechanisms have made people consider how to remedy the situation for a long time. Yet, it has been the recent rise of new microcredit and –saving concepts that has led governments, private banks, NGO's and mutual benefit associations to the question: can we apply these new concepts to insurance, designing products especially suited for poor clients? In this spirit, many microinsurance initiatives are currently being launched and several pilot schemes are already in the field. Despite this effort, demand for microinsurance is still very low (Cole et al., 2009; Giné & Yang, 2007; Ito & Kono, 2010) and practitioners are working to implement an affordable insurance design that complements traditional informal risk sharing schemes. However, the potential effect of formal microinsurance on informal mechanisms is still an open question.

There are good reasons to believe in potential reductions of solidarity and an even negative overall effect of imperfect formal microinsurance on economic stability under certain circumstances (e.g. dependent on the fraction of insured people, strength of network, etc.). Crowding-out of pro-social behavior by market-based mechanisms is a well established effect in the economic and psychological literature (Bowles, 2008) and formal insurance can be seen

[2] The ILO Micro Insurance Compendium (Churchill, 2006, p. 34) also mentions informal group-based mechanisms (burial societies etc.) as informal insurance. However, this already is a step towards formal mechanisms with an explicit obligation to pay contributions in order to receive benefits, and we refer to more flexible and non-contractual arrangements.

[3] Morduch (1999) summarizes some literature and concludes: "Most informal insurance mechanisms are typically weak and often provide only inadequate protection to poor households" (Morduch, 1999, p. 188).

as such a market mechanism. Instead of relying on the intrinsic motivated solidarity payments individuals can pay a price to benefit from more security. At least two of the causes for crowding-out identified in Bowles (2008) may apply: First, people could perceive the availability of costly insurance as a signal that 'buying' security is everyone's own responsibility (framing effect). Second, the fact that other people choose insurance might hint at their low commitment or trust in the existing solidarity transfer scheme and provoke a negative response (information effect). The crowding-out effect might also lead to individualization and the breaking apart of traditional structures which also affects other spheres of life.

There is already some literature specifically suggesting that insurance might crowd out solidarity transfers within the network. However this literature is either theoretical (Attanasio & Rios-Rull, 2000) or is based on non-experimental data and transfers are not measured directly (Dercon & Krishnan, 2003; Jowett, 2003).[4] In observational studies it is also not possible to disentangle the process leading to the crowding-out effect. Are the insured reducing their transfers or the uninsured? What motives drive peoples' decision?

This paper delivers the first experimental evidence on whether informal solidarity is reduced by formal insurance in developing countries. Our design tries to reflect reality as much as possible. We model risk in a behavioral game using lotteries that involve rolling a dice. Every participant is provided with an initial endowment and depending on the dice roll she is allowed to keep all or part of it. Informal risk-sharing is implemented in groups of three according to the design of Selten & Ockenfels (1998). After the lottery is played, each group member can transfer some money to the other group members. Insurance is introduced via offering alternative lotteries that are safer but require some ex-ante fixed payment. Since acts of giving are not always voluntary but are "demanded" from the network members (Comola & Marcel Fafchamps, 2010; Grimm et al., 2010), we implemented the possibility of secretly saving money in a lockbox treatment where players can pretend to have a medium shock and hide their money from the risk sharing group. We test two variations of insurance; one protects only against catastrophic losses and the other is a more comprehensive type and covers more cases. In contrast to real world data the controlled environment of the behavioral game allows monitoring the transfers and choices of participants perfectly and thus delivers

[4] Attanasio & Rios-Rull (2000) present a mathematical model explaining that under some conditions public insurance leads to a decrease in informal transfers. Dercon & Krishnan (2003) and Jowett (2003) present empirical evidence in line with crowding out of private by public support schemes. However, all evidence is based on non-experimental data and transfers are not measured directly, amongst others. The research is made more difficult by the fact that the measurement of informal solidarity and transfers is a difficult task. Comola & Fafchamps (2009) for example use data where the receiver and sender both report on transfers. They show that the information from the two parties is largely inconsistent.

much more reliable empirical results. Our main findings are that 1) there is evidence that the availability of insurance reduces solidarity and 2) this negative effect might even persist if insurance is removed.

The remainder of the paper is organized as follows. Section II describes the experimental setup including treatments, hypotheses, implementation and subject pool. We discuss empirical results in section III and conclude in section IV.

II. Setup of the experiment

We benefited from the work of Barr & Genicot (2008) who also combine a lottery choice with risk-sharing after the result is determined.[5] However, in their experimental procedure, lottery choice is not a treatment, so the effect of introducing insurance cannot be identified. Also, interpretation of the many gamble choices (according to Binswanger, 1980) as different insurance products is difficult. There are other experiments that come closer to our idea. Trhal & Radermacher (2009), for example, compare solidarity in treatments with and without gamble choice. Yet, the 'non-insurance' lotteries are not the same in both treatments and some other details do not fit our purpose. We consequently designed a novel behavioral experiment that is described in the following.

We model risk in a behavioral game using lotteries that involve rolling a dice. Every participant is provided an initial endowment of 200 Philippine Pesos (PhP) and depending on the dice roll she is allowed to keep all or part of it.[6] This design reflects the risk to lose money instead of providing participants with the possibility of winning money.[7] Informal risk-sharing is implemented in groups of three according to the standard solidarity game procedure (Selten & Ockenfels, 1998).[8] After the lottery is played, the group is allowed to talk and then each member of the group can transfer some of his money to the other group members.

[5] They tested different enforcement mechanisms in their experiment and find strong evidence for intrinsic motivation of giving, as substantial risk sharing takes place even if individuals can secretly opt out of the solidarity group.

[6] The amounts where such that the expected payoff of participating in the experiment (237 PhP, or about 5-6 USD) equals about one day of minimum wage in the formal sector. The expected amount includes a show-up fee of 100 PhP that every participant received for sure.

[7] Harrison & Rutström, (2008: 90) stress the importance of the reference point, referring to prospect theory (Kahneman & Tversky, 1979) that allows subjective probability weighting, a reference point and different utility functions for losses and gains.

[8] There are problems with the 3-player approach since often winners do not anticipate that the other winner might also give. This leads to the strange situation that the player with the worst shock leaves the experiment with the highest earning. However, this happened only eight out of 279 times. We also believe that two player relations are different from risk-sharing groups and thus not adequate for our experiment.

Insurance is introduced via offering alternative lotteries that are safer but require some fixed payment ex ante.

- Treatments -

We test *two variations of insurance*, compared to no-insurance. One insurance protects against half of all loss types and is more expensive, while the other insurance covers half of catastrophic shocks only.

We fist explain the no-insurance treatment, which we refer to as Option A. Every participant has an initial endowment of 200 Philippine Pesos. This is the amount earned if the dice roll gives a 1, 2 or 3, i.e. no shock (no loss). If the dice shows a 4 or 5 a medium shock occurs (losing half) and a 6 means a catastrophic shock (losing almost everything). If the medium shock occurs participants lose 100 of their initial 200 Pesos and if the catastrophic event occurs they lose 180. In case of no shock, participants do not lose any money, but can keep all their 200 Pesos.

The two insurance variants are called option B and option C.[9] For option B participants have to pay 45 Pesos in advance and half of all losses are covered. The price for option C is only 20 Pesos, but half of only the catastrophic loss is covered. (The prices 20 and 45 are chosen to reflect the higher administrative effort of more comprehensive insurances in reality. The more comprehensive insurance covers more shocks and is therefore confronted with more claims, and also a higher administrative effort, which translates to lower expected payoffs.) Table 1 shows the payout for the no-insurance case and the two insurance options B and C.

The advance cost of insurance thus is always the 'guaranteed loss' in case of no shock. In general, different options lead to a different spread of payoffs; the lower the standard deviation, the lower the expected total payoff. Option B is most costly, not only regarding absolute price but also when looking at the expected loss. Yet the risk – as represented by standard deviation of the payoff – is smaller than in option A and C. Option C is an intermediate case with an interesting additional feature: Due to the low price and the focus on the catastrophic loss it can secure an even higher minimum payoff than option B. Because of this, individuals with minimax preferences would prefer C over B. Both options B and C reflect typical insurance products where full coverage is impossible. E.g. in most developing countries, health insurance covers only the medical expenses (often below 100%), but not lost income due to lost labour. The more comprehensive insurance mimics the state owned

[9] We would have expected a higher crowding-out effect by labelling the lotteries as "insurance" instead of "option" but decided to leave this for future research.

medical insurance scheme and the catastrophic insurance reflects different rainfall or crop insurance in the region. With two insurance products, we are able to discuss demand for different insurance products and create a different take-up which might lead to more or less crowding-out. We are also able to detect product inherent effects that interact with solidarity.

Table 1: Payoffs (in PhP) under different (insurance) options

Dice Result	1,2,3: no shock	4,5: med. Shock	6: cat. shock	Std [Loss]	E [Loss]
Option A	**- 0** = 200	**- 100** = 100	**- 180** = 20	68.7	-63.3
Option B	**- 45** = 155	**- 95** = 105	**- 135** = 65	34.4	-73.7
Option C	**- 20** = 180	**- 120** = 80	**- 110** = 90	48.5	-68.3

To test the effect of introducing insurance and the differential effect of the two types, the behavioral experiments were implemented as outlined in Table 2. In six villages (treatment block A) no insurance is offered in round one and two. Hence, participants have no choice and always play option A. In round three both insurance types are introduced and participants can chose between all three options. In eight villages (treatment block AB), insurance option B is offered in round one, no insurance in round two and again insurance option B in round three. In another eight villages (treatment block AC), the same is done with option C. Note that each group plays all three rounds, but for only one of the three rounds a payout takes place. The round that is being paid out is randomly chosen after all three rounds have been completed. The participants knew this in advance. Hence, apart from possible learning effects, no dynamic, strategic or endgame effects can occur.

Table 2: Treatment plan for insurance types

	Block A (6 villages)	**Block AB (8 villages)**	**Block AC (8 villages)**
Round 1	Option A	Choice: Option A or B	Choice: Option A or C
Round 2	Option A	Option A	Option A
Round 3	Choice: Option A, B or C	Choice: Option A or B	Choice: Option A or C

Our main hypotheses are:

(I) Solidarity transfers are reduced by the availability of insurance.

(II) There is a persistent reduction of solidarity even if insurance is removed.

The effect of the different insurance types on separate markets can be tested by comparing treatment block A versus block AB versus block AC in the first round (Hypothesis I). Treatment A serves as a control here. The persistent effect of insurance on solidarity can be tested in the second round (Hypothesis II). The third round allows for a comparison of demand for the insurance variants while competing in one market in block A. Additionally, it is possible to compare take-up of option B and option C with some circumstances changed.[10] More generally, the third round also delivers more observations for the pooled regressions at a later stage.

- Additional treatments -

In real life, observing what everybody gives to you is normally unproblematic, but perfectly observing individual shock levels of others is maybe not possible. Thus we decided to allow participants to pretend a negative shock. Catastrophic losses might on the other hand be observable to everybody. Therefore, observability of medium shocks was reduced in a *saving* treatment. If the dice result was 1, 2 or 3 (no shock) individuals could decide to save the monetary difference to a medium shock in a secret lockbox. This information was private to the individual and group members were only told the amount the person had left after the lottery/lockbox stage. Saving in the lockbox thus made it impossible for the co-players to distinguish between no shock and a medium shock. The aim of this treatment is to increase external validity of our study and to show the effect of secret saving on solidarity. It is still possible for people with no shock to help others in case of need, but a lot of solidarity based on peer pressure will be reduced. Furthermore, *network strength* in the group was varied. Normally one randomly selected person had to invite two other household heads that he knows very well to join the experiment. In the endogenous treatment, the players who knew each other very well would form a solidarity group. In half of the villages, however, we implemented an exogenous treatment where groups were mixed up and participants would play with two random partners from their village.[11]

[10] In treatment blocks AB and AC the research team changed the ability to observe shocks (described below as an additional treatment) in some of the villages to test more hypotheses.

[11] Differences between the two types of groups can be found in participants' self-assessed relation to their group members. While more than 55% described their co-players as "close family" in endogenous groups, less than 30% do so in exogenous groups. Even though we do not believe that participants were very accurate in their

The analysis of these two additional treatments – secret saving and network strength – is not the core of this paper and a bias of the insurance effect due to their availability is impossible because they are balanced across insurance treatment blocks. However, there might be interactions between insurance and other treatments. Especially the availability of the saving box will be an important moderator of the insurance effect. We will therefore control for them in regressions and use them for sub-analysis.

- Implementation -

All participants were assigned to groups and received player numbers upon arrival. Depending on the network strength treatment, they would either remain in their group of three (see details on the recruitment below in the description of the subject pool) or they were randomly assigned to two new co-players. The groups stayed together for all three rounds and people in a solidarity group knew the other two members. After answering the pre-questionnaire, participants were seated to receive the introduction to the game. In an effort to make the rounds as independent from each other as possible, we made sure that signaling, punishment and the like cannot take place. Therefore decisions of co-players were not revealed and we did not allow for communication after the transfer choice. Group members were even seated separately to inhibit communication. The instructor pointed out that communication within groups is forbidden outside the communication stage, that violations of the treatment protocol will lead to the exclusion from the experiment, that three games will be played independently from each other and that only one of them will be paid out at random.[12]

The complete experimental procedure of one round is summarized in Figure 1. First, the instructor explains the game to all participants jointly, and everybody receives a plastic envelope with graphical instructions for this round and their initial endowment of 200 PhP in play money. Before participants go to a private room (1), they answer a set of questions to test their understanding of the game.[13] If the current round permits insurance options (see Table 2), participants are given a choice of lotteries. Otherwise only the standard lottery is available. After the participants make their lottery choice and pay the related price, they roll a dice to determine the loss. Where secret saving was available, players with no shock could then

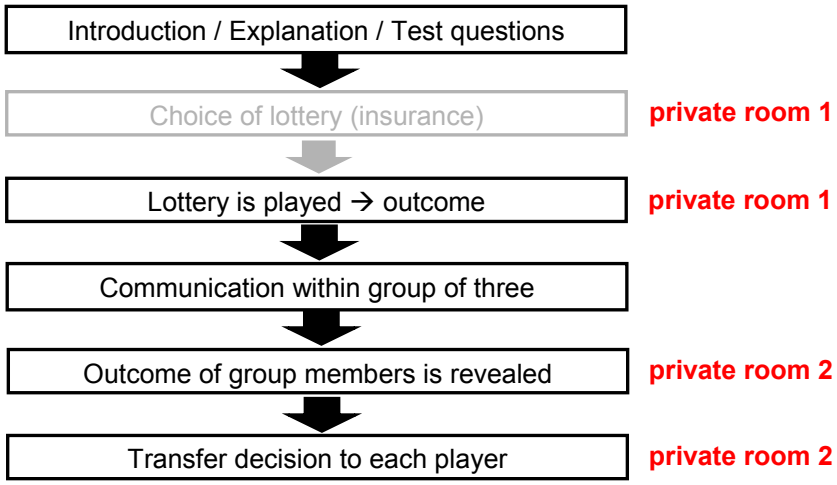
classification (30% seems very high for groups formed at random), the differences between the two types of groups is considerable.

[12] The amounts where such that the expected payoff of participating in the experiment (237 Philippine Pesos, or about 5-6 USD) equals about one day of minimum wage in the formal sector. The total amount includes a show-up fee of 100 PhP for every participant.

[13] The test questions can be found in the appendix. When participants made mistakes, the research assistants explained the setup once more. Only those who finally answered all questions correctly were allowed to participate, but fortunately we only had to exclude few participants.

decide to hide a fixed amount of their money. After all have chosen their amount to hide, the members are allowed to talk for approximately five minutes, before each individual separately goes to another private room (2). At this point, the amount that the two co-players took out of the first private room is revealed (endowment, minus insurance premium, minus loss due to shock, minus secret saving). Only the *net payout* is revealed, and not whether insurance has been bought, or whether shocks took place or whether secret savings have been made. From these payouts, however, one can induce who has purchased insurance and who not. The participant then decides if and how much to give to each of the co-players. Everybody is completely free in the way he or she shares the money. These transfers are never *revealed* to anyone. Hence, they cannot affect the behaviour in future rounds.

Figure 1: Experimental Procedure



To ensure that experimental conditions did not change, the same team of assistants was employed for the same job all the time, strictly adhering to the experimental protocol (i.e. the same person always read the protocol, the same assistants were sitting in room 1 and room 2). In the private rooms, decisions were recorded by the researchers. Communication within a group was restricted to the communication stage. Whenever there was an unclear situation, the researcher was present to decide on the issue. After all three rounds had been played, the completion of the post-game questionnaire and the random determination of which round to be paid out, the participants were handed out their winnings in *private*. All participants received a fixed show-up fee of 100 PhP in addition to their payoff from the relevant round.

- Subject pool -

The experiment was conducted in the Western Visayas (Region VI), in the province of Iloilo. Existing databases suggest that the region is a slightly disadvantaged but not unrepresentative region within the Philippines.[14] A two-stage random sampling procedure was applied throughout. First, we randomly determined the experimental sites, and then we drew participants within the selected barangay (lowest administrative level on the Philippines and often comparable to a village regarding size and structure). The exact combination of treatments played in one location according to the treatment plan was also determined randomly, but the randomization had to pass a balancing test regarding village size across the treatments.

The target population consists of low-income households in rural or partially urban areas. We therefore drew a random sample of 22 barangays whereby municipalities from the first income class (high income) and urban locations were excluded from the sampling process.[15] Also very small (population below 500) and very big (population higher than 3000) barangays were not considered to make the sample more homogenous.[16] Permission of the Punong Barangay (elected village representative) to conduct the research was obtained in all but one barangay, leading to its replacement by another random site. We made all possible effort to visit also remote locations, and all 22 locations of the sample could finally be reached.

In the second sampling stage, the households were randomly chosen within a barangay. Our recruiters went to the location some days prior to the experiment, asked the barangay officials for permission to run the experiment, ensured the availability of facilities for the games and requested a list of households from which eight households were randomly selected.[17] The recruiters then noted the names of the eight households and handed out invitation letters to them. Only the household head or the spouse of a household – in special cases also adult children still living in the household – were allowed to take part in the game. We also checked with the Punong Barangay whether the invited household representatives are too old to participate.[18] Each invitation had two additional letters attached as well as the

[14] The Demographic and Health Survey 2008 and a household survey conducted by the University of Mannheim in 2009 that is available to the authors suggest the following: educational attainment is slightly below national average, poverty is higher and coverage with public health insurance is around average.
[15] Income Classification based on Department of Finance Department Order No.20-05 Effective July 29, 2005 (source: http://www.nscb.gov.ph).
[16] Four of the 22 brangay were already chosen at random for an earlier household survey. To link the data from both studies they were included even though one barangay was slightly too small (350) and another one slightly too large (3123).
[17] Every barangay was able to provide a complete household list.
[18] Our preferred age was between 18-60 years, but we mainly relied on the judgement of the Punong Barangay regarding the fitness of participants. Participants above age 70 are not considered, though.

instruction to invite representatives from two more distinct households by choice. So the maximum was 24 participants per experimental site.

Table 3: Descriptive statistics of participants

	All (N=466)				A (N=132)	AB (N=167)	AC (N=167)
Variable	Mean	Std.	Min	Max	Mean	Mean	Mean
Male	0.31		0	1	0.3	0.29	0.35
Household head	0.31		0	1	0.24	0.3	0.37**
Married	0.81		0	1	0.83	0.8	0.8
Highschool education	0.44		0	1	0.49	0.48	0.37*
College education	0.25		0	1	0.23	0.3	0.21
Age (in years)	42.7	12.13	18	69	42.7	41.2	44.2
Regular monetary income? (dummy)	0.23		0	1	0.23	0.25	0.22
Skip meals in last month	0.3		0	1	0.3	0.23	0.35
In debt with more than 1000 Pesos?	0.57		0	1	0.55	0.64	0.51

Stars give significance level of Wilcoxon ranksum test for differences to mean in treatment A
*** p<0.01, ** p<0.05, * p<0.1

Descriptive statistics of the participants are presented in Table 3. Most of them are female (69%), and therefore the share of household heads is only 31%. Educational level is relatively high with more than two thirds having attended at least high school. Below 18 year olds were not allowed to take part in the game and senior individuals with 70 years and above are excluded from the analysis. Less than a quarter report regular monetary income. 30% of households have members that had to reduce meals for financial reasons, which serves as a measure of poverty. 57% are in debt with more than 1000 Pesos, the equivalent of roughly 22 US dollars.[19]

Due to the randomized assignment to treatments, we expect that all characteristics should be balanced, but in reality some small-sample correlation can remain. For example there is a higher share of household heads in treatment AC than in the control A and the educational attainment is slightly lower. The same is true for village characteristics, shown in Table A1 of the appendix. Especially income class of the municipality is somewhat different by chance across treatment blocks. Otherwise most characteristics are balanced. Nevertheless, the small-sample correlation in some characteristics hints at the importance of controlling for covariates in regressions.

[19] Around half of them owe the money to friends or relatives.

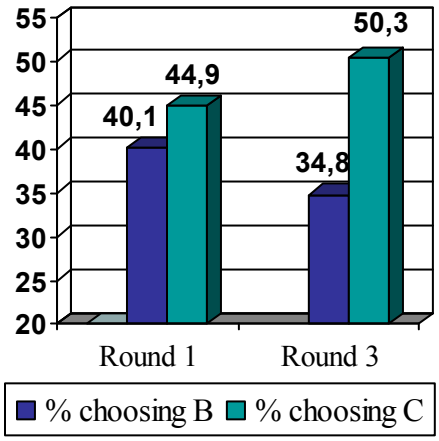
III. Empirical Results

In the following, we will first consider some descriptive results on the effect of insurance using the comparisons implied by the treatment plan (compare Table 2). Afterwards we will control for different shock distributions across treatments via matching, before employing a parametric regression model to control for more potentially confounding covariates and to gain further insights.

- Descriptive results -

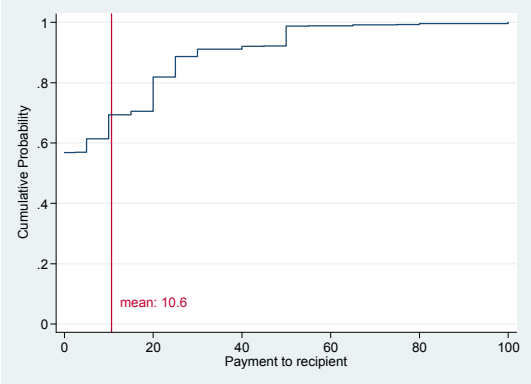
The safe lottery options are demanded by participants. On average 46% 'buy' insurance if they have the possibility to do so. Figure 2 illustrates the demand by treatment and round, and shows that lottery type C is more popular than type B, especially in the last round. This is also reflected in the retention rates from round one to three (B: 57%, C: 72%).[20] Transfers between group members vary greatly between 0 and 100, with a mean of 10.6 pesos. Figure 3 displays the distribution of the 2730 observed payments from sender to recipient.[21] In 57% of all cases transfers are zero. Standard deviation is 16.5 pesos and the mean is 10.6 pesos. Regarding the secret saving, an overwhelming majority uses the device. In about 94% of cases when saving is possible participants decide to do so.[22]

Figure 2: Demand for insurance on separate markets



Note: only treatment blocks AB and AC (without block A, round 3)

Figure 3: Cumulative distribution of transfers



[20] Figure 2 focuses on treatment block AB and AC where different insurances are offered on separate markets. When type B and C compete in one market (treatment block A in round three) demand for type C (43.4%) is a clearly higher than for type B (17.8%).
[21] Each participant of the 466 makes two transfer decisions per round. One group dropped out in round two and another group in round three.
[22] Remember that participants can only save and thus pretend a medium shock if they have no shock.

The transfers are described in a compact form in figure 3; however they do not necessarily indicate effective redistribution of the money, as every group member can transfer to the other *and vice versa*. Let T_{ij} be the transfers from player i to j. Real redistribution is the result of *net* transfers, that is transfers from player i to j minus transfers from j to i ($T_{ij} - T_{ji}$). Therefore it will not be enough to compare average transfers across treatments, as they might simply reflect a different inclination to give in general, which is completely irrelevant for redistribution.[23] Solidarity works (in the sense of risk sharing) if the better-off give more to the worse-off than the other way around. For the descriptive analysis of the treatment effects let us therefore start with a comparison of net transfers across treatments - of those with a less severe shock than the potential recipient. This means we only look at net redistribution from those without a shock to co-players with at least a medium shock, or of those with a medium shock to co-players with a catastrophic shock.

Table 4: Net transfers from group members with less severe shocks than recipients

		All		Block A	Block AB vs. A	Block AC vs. A
Round	Variable	Mean	Std.	Mean	Difference	Difference
1	Net Transfer to recipient	8.6	20.8	10.9	-1.1	**-5.2***
	N	270		68	107	95
2	Net Transfer to recipient	15.1	23.5	16.1	+2.2	-4.4
	N	282		69	101	112

Stars give significance level of Wilcoxon ranksum test for differences to mean in treatment A
*** p<0.01, ** p<0.05, * p<0.1

Table 4 shows the average net transfers to the disadvantaged co-player by treatment block and round. Net transfers in treatment blocks AB and AC are shown relative to block A. Remember that the comparison in the first round allows testing the effect of different insurance types on separate markets by comparing treatment block A with block AB and with block AC in the first round (Hypothesis I). Treatment A serves as a control. The persistent effect of insurance on solidarity can be tested in the second round (Hypothesis II). Also note that these comparisons across treatment blocks give the effect of insurance availability, not of take-up.

On average, participants distribute 8.6 pesos in the first round and 15.1 pesos in the second round from the better-off to the worse-off. Solidarity thus seems to work in tendency. However, net transfers vary greatly in both rounds which leads to high standard deviations.

[23] Imagine a treatment that leads *all* participants to give more. If this effect is the same for the better- and the worse-off, the two level effects will just cancel out after mutual transfers and redistribution is unaffected.

Still, a Wilcoxon ranksum test statistic shows significantly lower solidarity transfers at the 10% level when insurance type C is available. The differences in the second round, i.e. when no insurance is available in any villages, are not significant.

The effect becomes much more pronounced if we look at those villages where participants cannot secretly save money, this means where shocks are observable to other participants. Table 5 shows that the effects become larger and more significant. Here both treatment effects are significant at the 5% (AB) and 10% (AC) level, respectively, and have about the same size. Results suggest that net redistribution with insurance is only around half of what it would be without formal insurance. Also, availability of insurance C displays a marginally significant persistent effect in the second round. This is not the case for the more comprehensive type B. Repeating the exercise for those villages with the secret saving possibility leads to no significant differences (compare Table A2 in the appendix of this paper).[24]

Table 5: Net transfers from group members with less severe shocks than recipients (no saving)

		All		Block A	Block AB vs. A	Block AC vs. A
Round	Variable	Mean	Std.	Mean	Difference	Difference
1	Net Transfer to recipient	12.2	21.2	18.4	**-8****	**-9.3***
	N	141		40	52	49
2	Net Transfer to recipient	19.9	23.5	23.4	+0.6	**-9.2***
	N	130		37	47	56

Stars give significance level of Wilcoxon ranksum test for differences to mean in treatment A
*** p<0.01, ** p<0.05, * p<0.1

The problem with this kind of analysis is that the amount of redistribution is likely to depend on the degree of inequality in the group. Unfortunately the dice results within the groups are not guaranteed to be equally distributed across treatment blocks and rounds. This could seriously affect the validity of the above comparisons.[25]

- Matching results -

[24] Also note that net transfers from the better- to the worse-off are dramatically lower on average in this subset of the sample (4.7 pesos versus 12.2 pesos in round one, 10.4 versus 19.9 pesos in round two).

[25] Comparing the shock dispersion across treatments and rounds does indeed reveal differences in the shock dispersion that are significant at the 10% level in some cases. As this is a result of dice rolls, it is by definition pure chance and large differences should never be present in large samples. However, in our case this is a small-sample correlation that might nevertheless bias results. Figures are not shown here but can be obtained from the authors upon request.

A way to control for the potential problem of different shock distributions across treatments is via matching. If we have a sender and recipient with certain shocks in treatment block AB, we look for sender and recipient with the same shocks in treatment block A (controls). At the same time, we can also make sure that the third group member has the same shock, the round is the same and so on. However, we have to restrict our conditioning set to find enough comparable pairs. Table 6 shows the average treatment effect on the treated (ATT) of the two treatment blocks in round one and two using exact matching. The last two columns of Table 6 do the same for the subset of villages without the secret saving possibility.

Matching results largely confirm descriptive statistics. Availability of insurance type C is associated with lower solidarity transfers from the better- to the worse-off. When restricting attention to villages without the saving lockbox we see larger effects and the persistent effect of insurance type C becomes slightly significant. Effects are insignificant for the more comprehensive scheme B. Matching for the set of villages with a saving lockbox again does not show any significant effects (results not shown here).

Table 6: Average treatment effect on the treated (ATT) of treatment blocks on net transfers from group members with less severe shocks than recipients (all and without saving)

		all		no saving	
Round	Outcome variable	Block AB vs. A (ATT)	Block AC vs. A (ATT)	Block AB vs. A (ATT)	Block AC vs. A (ATT)
1	Net Transfer to recipient	-1.2	**-7.9****	-5.2	**-11.9****
	N (On/off support)	80 / 27	81 / 14	43 / 9	42 / 7
2	Net Transfer to recipient	+4.2	-3.9	-1.5	**-11.0***
	N (On/off support)	84 / 17	97 / 15	35 / 12	47 / 9

Stars give significance level of ATT using bootstrap standard errors, *** p<0.01, ** p<0.05, * p<0.1, exact matching on shock distribution, saving possibility and network strength.

Descriptive and matching results suggest a negative impact of insurance availability on solidarity (Hypothesis I), but only if there is no secret saving, i.e. shocks of co-players are observable. This is especially true for insurance option C. For this type there are even signs for a persistent effect if insurance is removed again (Hypothesis II). However, effects are not so clear for insurance type B. Also, regarding Hypothesis (I) we do not know whether the effect is due to a crowding-out of motives or simply because insurance reduces inequality and thus lowers the incentive to redistribute. It is not possible to answer this question using descriptive comparisons, as insurance necessarily implies a reduction in inequality. We will

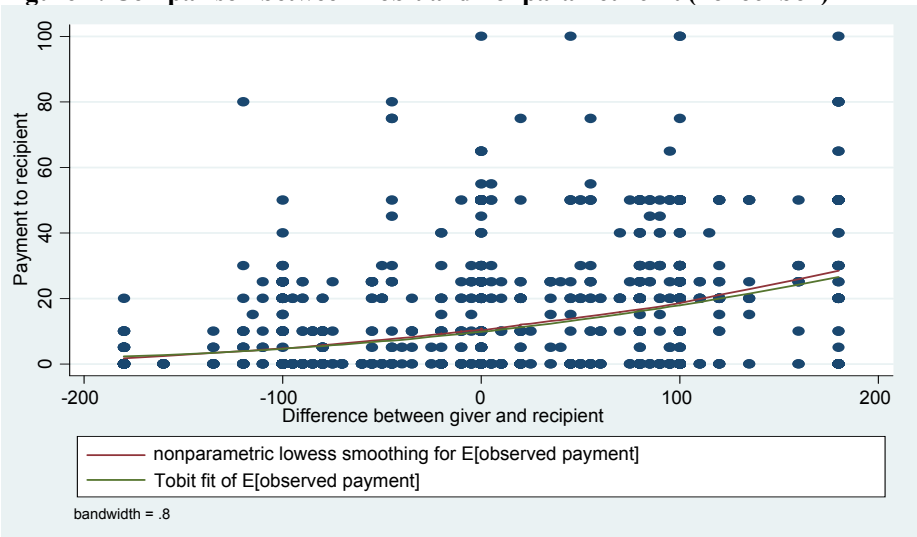
therefore turn to regressions to gain more insights before further discussing these effects and the circumstances under which they appear.

- Regression specification -

A way to disentangle the effect through reduced inequality and the additional effects of insurance is to use a parametric assumption on the dependence between inequality and transfers. Using a regression model, one can then control for the difference in inequality and attribute the remaining difference to crowding-out effects. There are two additional reasons why the simple descriptive statistics might not deliver good evidence. The first is that transfers vary so much that it is hard to detect significant differences in the statistical noise. The second reason is that important factors, like the dice results and individual characteristics, might be correlated with treatments due to limited sample size. Both support a regression approach to 1) eliminate possible bias due to confounding variables in the small sample and 2) to reduce the large unexplained variation in transfers by including important background characteristics. The choice of the regression model will be explained and motivated in the following.

Individual transfers are left-censored at zero as negative transfers are not allowed. Since ordinary linear regression models are not suited for this problem, we rely on Tobit regression with the main assumption that the latent willingness to give is proportional to the pre-transfer difference between giver and recipient. Figure 4 shows a nonparametric fit of the relationship and the parametric Tobit regression fit. The comparison reveals that the Tobit regression fits the main relationship very well, with only a slight divergence at the very right extreme.

Figure 4: Comparison between Tobit and nonparametric fit (no lockbox)



Regressions only use villages with observable shocks (no saving lockbox available).

In principle we are still interested in net transfers. However, net transfers are a result of the difference of two censored variables. Even if the underlying *latent willingness* is linear in regressors, the difference of the *observed transfers* will not be linear.[26] We thus have to separately model the transfers between partners. Consider the following regression model:

$$T_{ij}^{*} = \begin{pmatrix} Y_i - Y_j \\ Y_i \\ S_i \\ S_j \\ X_i \\ X_j \end{pmatrix}^{T} \beta_i + \varepsilon_{c,r} + \varepsilon_i \qquad \text{where } T_{ij}^{*} \text{ is the latent transfer from i to j}$$

and

$$T_{ij} = T_{ij}^{*} \qquad \text{if} \qquad T_{ij}^{*} \geq 0$$

$$T_{ij} = 0 \qquad \text{if} \qquad T_{ij}^{*} < 0$$

Latent transfers are influenced by the difference between both group members ($Y_i - Y_j$) and the level of income after the lottery (Y_i). Saved earnings of individuals i and j (S_i, S_i) may also influence latent willingness to give because they alter the observed difference between players. Individual level covariates of sender/recipient (X_i, X_j), community-round fixed effects ($\varepsilon_{c,r}$) and an individual error term (ε_i) are also allowed to affect T_{ij}^{*}. Note that all level effects that do not vary within a village-round cell are included in the fixed effect $\varepsilon_{c,r}$. This includes treatment and community fixed effects. However, these level effects are common to both the sender and the receiver and are thus not of immediate interest.

What is more interesting is the variation in β_i. If there is more solidarity of the better-off with the worse-off, transfers will be more sensitive to inequality. In other words, the β coefficient will be larger. By allowing different coefficients across treatment blocks and rounds we can capture treatment effects, at the same time controlling for real and observed differences as well as individual covariates of the sender/recipient.

- Regression results -

[26] An exception is the case when all regressors for T_{ij} are the negative of the regressors for T_{ji}. Linearity of the expected value could then be shown. This would for example be the case when the difference between sender and recipient was the only relevant explanatory, but we allow for many more influential factors.

Table 7 shows the results of Tobit regressions. Specification (1) simply regresses transfers on level of pre-transfer earnings (*RealMoney*), difference of earnings compared to the recipient (*Difference RealMoney*) and the amount saved (*Saving*) by sender and recipient. Regression (2) includes the fixed effects and specification (3) adds individual covariates of sender and recipient.[27] We can see that the difference in earnings between sender and receiver is an important explanatory variable. The effect is highly significant and suggests that for each peso of difference the latent willingness to give rises by an additional 0.13-0.14 pesos. If money is secretly saved by the sender, this substantially reduces the inclination to give. Conversely, savings of the recipient increase transfers, as differences appear more in favor of the sender. It seems that mainly observable differences drive redistribution.

Table 7: Tobit regressions explaining transfer

VARIABLES	(1) all	(2) all	(3) all	(4) all	(5) no saving	(6) saving
RealMoney	0.047	0.034	0.030	0.032	0.024	0.048
Difference RealMoney	**0.13*** **	**0.14*** **	**0.14*** **	**0.12*** **	**0.15*** **	0.088
Difference x Treat B				-0.020	**-0.084** **	**0.041** *
Difference x Treat C				-0.037	**-0.090** **	-0.0077
Difference x Treat B x Treat C				0.059	0.13	0.0033
Difference x PseudoTreat B				0.035	-0.021	**0.081** *
Difference x PseudoTreat C				-0.030	**-0.11** **	0.022
Difference x Round				0.019	**0.032** **	0.0065
Difference x Exogenous Group				-0.019	-0.016	-0.0071
Saving sender	**-0.11*** **	**-0.11*** **	**-0.13*** **	**-0.12*** **		**-0.13** **
Saving recipient	**0.11*** **	**0.12*** **	**0.12*** **	**0.11*** **		**0.099** **
Village-round fixed effects	NO	YES	YES	YES	YES	YES
Individual controls	NO	NO	YES	YES	YES	YES
Observations	2730	2730	2730	2730	1664	1066

Standard errors in parentheses, clustered at the village level
*** p<0.01, ** p<0.05, * p<0.1

Columns (1) to (3) assume that all participants react the same way to pre-transfer differences (β is constant). We can relax this assumption by including interactions with differences. For example, if we interact differences with *Treat B* (availability of insurance type B), the

[27] The individual covariates used in regression (3) and their effect can be seen in Table A3 of the appendix. It seems that men give more to others, age has a positive marginal effect at the beginning that becomes negative from around 47 on and being indebted is associated with giving more. Characteristics of the recipient are all insignificant except age where effects are similar but weaker than for age of the sender. These effects are all level effects and unrelated to differences in earnings.

reaction on inequality of those with insurance option B will be reflected in the coefficient of *Difference RealMoney* plus the coefficient of the interaction. In columns (4) to (6) of Table 7, we allow the β vector to vary by including interactions between real differences before transfers and the following treatment variables: (*Treat B*) is the availability of insurance type B [in treatment block AB in rounds 1 and 3, in block A in round 3], (*Treat C*) is the availability of insurance type C [in treatment block AC in rounds 1 and 3, in block A in round 3], (*Treat B x Treat C*) is the availability of insurance type B and C [in treatment block A in round 3], (Pseudo*Treat B*) is the availability of insurance type B in the previous round [in treatment block AB in round 2], (Pseudo*Treat C*) is the availability of insurance type C in the previous round [in treatment block AC in round 2], (*Round*) is the round number and (*Exogenous Group*) is one if group was formed at random.

Specification (4) in Table 7 shows the results of letting β vary. Difference in pre-transfer earnings and savings of sender/recipient are again highly significant and similar in size to before, but none of the interactions displays a clear effect. However, if we focus on the villages without secret saving possibility (specification 5) an interesting picture emerges. Availability of both insurance types reduces the variability of transfers with observed differences by more than half. The effects are both significant at the 5% level. Also, the significantly negative interaction term with (*PseudoTreat C*) suggests a persistence of the effect even if insurance is removed in the second round. As in the descriptive comparisons and for the matching results, no persistence of type C can be found. Below we run several robustness checks to assess the robustness of these results. Specification (6) only considers villages with the secret saving possibility. Effects are less clear and the only two significant effects prove to be not robust when repeating the same checks as for non-saving villages.[28]

As a robustness check for the above result we repeat the regression (using only non-saving villages) with more controls and in different subsamples. Results are summarized in Table 8. First, we add more interactions between pre-transfer differences and individual/village covariates (1). Then we exclude individuals with a lower level of understanding according to our test questions or particularly 'irrational' transfers (2).[29] We also restrict our sample to only round one and two (3) and round two and three (4), respectively. Results show that the effect of insurance treatment B and C always goes in the right direction and is mostly significant. The effect of type C even appears to be persistent if availability is removed (*PseudoTreat C*). The related coefficients are highly significant across

[28] Results are not shown here, but are available from the authors upon request.
[29] Irrational means individuals that lost more than some other group member, but still transferred more than 40% of their money and got less from the others than what they gave.

almost all specifications.[30] Specification (5) shows how the number of insured in a group influences transfer patterns. The interaction between pre-transfer differences and the number of insured in the group is significantly negative for type C. This means, solidarity – in the sense of reaction to inequality – is lower in groups with more insured members.

Table 8: Tobit regressions explaining transfer (villages with no saving) – further checks

			subsets		
VARIABLES	(1) More interact's	(2) High understand	(3) Round 1+2	(4) Round 2+3	(5) Number insured
RealMoney	0.026	0.033	0.020	0.044	0.031
Difference RealMoney	0.095	0.11	0.12	0.099	0.100
Difference x Treat B	**-0.079***	**-0.11****	-0.080	-0.078	**-0.078***
Difference x Treat C	**-0.085****	**-0.091***	-0.028	**-0.11****	-0.064
Difference x Treat B x Treat C	**0.13***	**0.14***		**0.16***	**0.14***
Difference x PseudoTreat B	-0.0011	-0.023	-0.012	-0.033	-0.0050
Difference x PseudoTreat C	**-0.11*****	**-0.14*****	**-0.12****	**-0.14*****	**-0.10*****
Difference x Round	**0.025***	0.015	0.054		**0.027****
Difference x Exogenous Group	0.0085	0.015	-0.011	0.0034	0.011
Difference x Income Class	0.039	0.043	0.013	**0.061****	0.038
Difference x Partially Urban	**-0.075****	-0.054	-0.040	**-0.079***	**-0.081****
Difference x Higher Education	-0.034	-0.018	-0.032	-0.035	-0.033
Number Insured B in Group					4.45
Number Insured C in Group					-0.87
Difference x Number Insured B					0.0026
Difference x Number Insured C					**-0.026***
Village-round fixed effects	YES	YES	YES	YES	YES
Individual controls	YES	YES	YES	YES	YES
Observations	1664	1234	890	1216	1662

Standard errors in parentheses, clustered at the village level
*** p<0.01, ** p<0.05, * p<0.1

Overall, empirical results suggest that there is a negative effect of insurance on solidarity if there is no secret saving possibility. Under these circumstances, the effect of the catastrophic-only insurance is persistent even if insurance is removed. Furthermore, additional robustness checks show that the effect of the catastrophic-only insurance is more robust and persistent than the effect of the comprehensive insurance type B. Regressions controlling for insurance

[30] Further robustness checks, for example expanding the interactions with community and individual characteristics, shows that the negative effect of the catastrophic-only insurance (*Treat C*) is more robust than the effect of the more comprehensive type B (results not shown here).

take-up hint at the importance of the prevalence of insurance for the overall effect, so the stronger and more persistent effect of type C could be due to higher 'acceptance' amongst participants.

IV. Conclusions

We have conducted a novel behavioral experiment with rural and partially urban villagers on the Philippines. This experiment – simulating a risky environment with solidarity networks and the introduction of insurance – delivers the first experimental evidence on whether informal solidarity is reduced by formal insurance in developing countries. Our data confirms that the availability of insurance reduces solidarity and that this negative effect might even persist if insurance is removed. Regressions reveal that the negative effect is not only due to lower inequality between those with insurance but that there is an additional crowding-out effect on solidarity. However, this is only the case if shocks of network members are observable. Also, the evidence is much stronger for insurance focusing on catastrophic shocks as compared to insurance for all types of shocks.

So why do effects only exist when shocks are observable? One important observation at this point is that the overwhelming majority secretly saves money and simulates a medium shock if there is the possibility to do so. As a consequence solidarity transfers are reduced dramatically (compare Table 5 and Table A2 in the appendix). With solidarity transfers being so low, observing further reductions is hard. This might very well explain why the insurance effect is focused on the non-saving villages where there is still solidarity in place.

Regarding the stronger negative effect of the catastrophic-only product we find that the impact increases with the number of insured in the group. At the same time, we observe considerably lower take-up of the more comprehensive scheme. Thus, participation seems to be too low to induce a 'common sense' that the market mechanism should apply. Some speculation allowed, this might have to do with the relatively high price of the comprehensive product. While everybody with reasonably high risk aversion can be expected to purchase the catastrophic-only insurance this is not the case for the more expensive version.[31]

[31] A simple simulation using a constant relative risk aversion utility fuction [$u(c) = (c^{1-\rho})/(1-\rho)$] shows that in the absence of solidarity a much larger share should choose insurance in treatment AC [risk-aversion parameter $\rho > 0.34$] than in treatment AB [risk-aversion parameter $\rho > 0.65$]. Note that $\rho > 0$ for risk-averse individuals.

In sum, our experimental results suggest that the introduction of insurance in solidarity networks might have unintended consequences under some circumstances. Especially if the network is able to observe the cash flow of members and reciprocal solidarity works well, these effects have to be taken into account. Short- and long-run effects are in line with the general literature on crowding-out of pro-social behavior by market based mechanisms (Bowles, 2008). Nevertheless, formal insurance might have considerable positive net effects, because informal solidarity is not always very effective. If network members can simulate shocks and thereby retreat from their solidarity commitment, availability of formal insurance can be a considerable improvement for individuals.

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Appendix I: Tables

Table A1: Descriptive statistics of villages

	All (N=22)				A (N=6)	AB (N=8)	AC (N=8)
Variable	Mean	Std.	Min	Max	Mean	Mean	Mean
How many people live in this community?	1264	653	350	3123	1445	1284	1109
different religious groups in this barangay	2.45	1.26	1	5	2.67	2.5	2.25
HHs with family members abroad (percent)	9.19	8.96	0	34.48	7.52	12.98	6.67
Conflicts between people	1.50	0.67	0	2	1.33	1.88	1.25
number of village organizations	7.23	1.66	4	11	7.83	6.88	7.13
People are selfish	6.36	2.97	0	10	5	7.88*	5.88
trust to lend/borrow	5.27	3.18	0	10	5	5.38	5.38
always somebody willing to help	7.95	2.36	0	10	8.17	8.13	7.63
Income Class	3.45	0.60	3	5	3	3.75**	3.5**
1=partially urban / 0=rural	0.68		0	1	0.5	0.75	0.75

Stars give significance level of Wilcoxon ranksum test for differences to mean in treatment A
*** p<0.01, ** p<0.05, * p<0.1

Table A2: Net transfers from group members with less severe shocks than recipients (with saving)

		All		Block A	Block AB vs. A	Block AC vs. A
Round	Variable	Mean	Std.	Mean	Difference	Difference
1	Net Transfer to recipient	4.7	19.7	0.2	+9	+1.9
	N	129		28	55	46
2	Net Transfer to recipient	10.4	22.6	7.7	+5.8	+1.5
	N	142		32	54	56

Stars give significance level of Wilcoxon ranksum test for differences to mean in treatment A
*** p<0.01, ** p<0.05, * p<0.1

Table A3: individual covariate coefficients for specification (3) of Table 7

Coefficients for characteristic of…	Sender	Recipient
Regular income?	**4.20***	0.92
Skip meals last month	-1.21	0.42
Debt > 1000 Pesos?	**4.30***	-0.15
Gender (0=fem, 1=male)	**9.96****	-1.93
HH head	-0.0098	0.37
Male x HH head	-3.77	2.17
Married	3.62	0.99
Highschool	-0.17	-0.39
College	0.26	3.85
Age	**2.17*****	**0.87****
Age squared	**-0.023*****	**-0.0074***
Village-round fixed effects	YES	
Observations	2730	

Standard errors in parentheses, clustered at the village level
*** p<0.01, ** p<0.05, * p<0.1

Appendix II: Example Test Questionnaire

(Notes: Example for treatment block AB, round 1, with saving. In reality we called option A "Angola", B "Botswana" and C "Cameroon" to avoid a notion of order in the options. Correct answers given.)

When do you decide which option you choose?

- [x] 1 before you throw the dice
- [ ] 2 after you throw the dice
- [ ] 3 whenever you like

CORRECT? YES ☐ NO ☐

Is the option BOTSWANA for free? YES ☐ NO ☒

CORRECT? YES ☐ NO ☐

How much does the option BOTSWANA cost? 45

CORRECT? YES ☐ NO ☐

How much do you have left if...

	With option BOTSWANA	With option ANGOLA
... you roll a 1?	155	200
... you roll a 2?	155	200
... you roll a 3?	155	200
... you roll a 4?	105	100
... you roll a 5?	105	100
... you roll a 6?	65	20

CORRECT? YES ☐ NO ☐

---------- ONLY IF WITH LOCKBOX ----------

When can you put money in the lockbox? Can you put money in the lockbox if you choose option ANGOLA and...

... you roll a 1?	YES ☒	NO ☐	If yes, how much	100
... you roll a 2?	YES ☒	NO ☐	If yes, how much	100
... you roll a 3?	YES ☒	NO ☐	If yes, how much	100
... you roll a 4?	YES ☐	NO ☒	If yes, how much	
... you roll a 5?	YES ☐	NO ☒	If yes, how much	
... you roll a 6?	YES ☐	NO ☒	If yes, how much	

CORRECT? YES ☐ NO ☐

When can you put money in the lockbox? Can you put money in the lockbox if you choose option BOTSWANA and...

... you roll a 1?	YES ☒	NO ☐	If yes, how much	50
... you roll a 2?	YES ☒	NO ☐	If yes, how much	50
... you roll a 3?	YES ☒	NO ☐	If yes, how much	50
... you roll a 4?	YES ☐	NO ☒	If yes, how much	
... you roll a 5?	YES ☐	NO ☒	If yes, how much	
... you roll a 6?	YES ☐	NO ☒	If yes, how much	

CORRECT? YES ☐ NO ☐

Will your group members know if you put money in the lockbox? YES ☐ NO ☒

CORRECT? YES ☐ NO ☐